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Michael R. Hill
University of Nebraska-Lincoln, michaelhilltemporary1@yahoo.com

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THE PRE-PARADIGMATIC IDEOLOGY OF EXPLAINED VARIANCE IN SOCIOLOGY

Michael R. Hill

Department of Sociology
University of Nebraska-Lincoln
Lincoln, Nebraska 68588-0324

1. Introduction and Assumptions

This discussion was originally conceived for presentation in one of the several MSS sections on **quantitative** methods. My project, however, was the only one for which the organizer of the quantitative sessions could not find a slot.[1] In fact, he neither told me my proposal was rejected nor communicated my name or abstract to the MSS program chair. It matters to me little whether this was by design or accident -- the result was the same. Were it not for Susan Wright's active response to my belated inquiries about the status of my participation in these meetings -- and her timely organization of this section -- the ideas outlined below would have been effectively squelched. I have re-worked my discussion, given its scheduling in a session on **qualitative** methods, and hope I raise at least a few points worthy of your time, consideration, and critical comment.

In addressing this discussion to the MSS session on **qualitative** methods, I assume that as sociological researchers we are neither untrained in quantitative techniques nor tremble timidly at the prospect of marshaling and analyzing data statistically when we deem it appropriate to do so. I assume also that we are dedicated to the canons of science, i.e, to conceptual rigor, careful data collection, intellectual and ethical integrity, and the advancement of knowledge through scholarly writing and teaching. I assume, in particular, your familiarity with the logic and mechanics of regression analysis in its simple and advanced forms, and that you have given Thomas Kuhn's (1970) **The Structure of Scientific Revolutions** more than cursory attention at some point in your studies.

In brief, I suggest, not only (as have many others) that the explanatory logic of regression analyses in sociology is flawed (see especially Stephen Turner 1987a,

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1987b, and the related symposium on "causality" in Sociological Theory), but also that the explanatory failure of regression analyses is widely and empirically obvious. In a mature or paradigmatic science, this latter situation should lead to the reform of sociological research, but has not. Why do systematic empiricists (to use David and Judith Willer's [1973] apt term) persist in using an interrelated set of analytic methods (based on the general linear model) that do not work? My thoughts along these lines were sparked recently when a noted philosopher of science, Professor Wesley Salmon (1989), spoke at the University of Nebraska on the topic, "Rationality and Objectivity in Science." Salmon's discussion (and reply to my questions) led me to conclude (1) that explained variance in sociology is typically ideological rather than scientific and (2) that quantitative sociology is at best a pre-paradigmatic activity rather than a mature science.

2. Paradigms and Pre-paradigmatic Attributes

A key point in this discussion is whether sociology is paradigmatic and therefore engages in the mature, standardized puzzle-solving activities that Thomas Kuhn (1970) calls "normal science." Starting with the publication of Kuhn's provocative book, first published in 1962, infatuation with "paradigms" by sociologists has run rampant to the point of absurdity.[2] Perhaps best known is George Ritzer's (1975) definition of sociology as a "multiple paradigm science."[3] Less well known is Lee Harvey's (1982:85) contorted attempt to show that sociology is "multi-paradigmatic in the very same sense Kuhn referred to it as pre-paradigmatic." Never mind that, for Kuhn, a paradigm is a **single**, highly integrated set of fundamental theoretical and methodological presuppositions to which **everyone** in a scientific discipline, such as physics, gives allegiance during the relatively long and stable periods of scientific work that Kuhn terms "normal science."

I was astounded a couple of years ago to hear Mayer Zald take credit for having originated the "paradigm of resource mobilization" and again recently when William Wilson reported that his approach to "the underclass" and "the truly disadvantaged" gave "a new paradigm" to the field of race relations.[4] In short, sociologists have now watered down Kuhn's lucid formulation to mean little more than a programmatic idea or theory sketch in which are few people in some sub-sub-specialty of sociology are momentarily interested.

Not everyone is bamboozled. Gerard Postiglione and Joseph Scimecca (1983: 179) concluded that paradigmaticism in

sociology is "an ideological invocation which in itself is a manifestation of the crisis in sociological explanation." Given this, note that Paul Roth (1984: 225) observed that social scientists ever since Weber repeatedly ask the question:

> "Are the social sciences really sciences?" . . . . the fact that social scientists persist in asking this question may be reason to doubt that they are convinced by their affirmative answers.

With rampant paradigmaticism, however, one simply invokes paradigm status for his/her work, whatever it is, and thereby claims scientific legitimacy **via** an implied reference to Kuhn's analysis of the history of physics. Using the ideological rhetoric of so-called "multiple paradigms," many sociologists avoid self doubt and stop asking embarrassing questions of themselves: they simply proceed, without philosophical warrant, **as if** they have a science.

To apply Kuhn appropriately, we must label sociology as "**pre**-paradigmatic." In physics, at least, the pre-paradigmatic setting was marked by many competing schools of thought. With regard to optics, wrote Kuhn (1970: 13):

> . . . anyone examining a survey of physical optics before Newton may well conclude that, though the field's practitioners were scientists, the net result of their activity was something less than science. Being able to take no common body of belief for granted, each writer . . . felt forced to build his field anew from its foundations. In doing so, his choice of supporting observation and experiment was relatively free, for there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. . . . That pattern is not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and invention. It is not, however, the pattern of development that physical optics acquired after Newton and that other natural sciences make familiar today.

Kuhn's description of pre-Newtonian physics is very close to contemporary sociology. We may be scientists, and we may even make significant discoveries, but we do not possess a discipline-wide paradigm. As a consequence, sociology lacks the characteristics of what Kuhn calls "normal science," i.e., the day-to-day, run-of-the-mill, routine puzzle solving and paradigm refinement of a mature scientific disciplines.[5] Hold on, for the moment, to the notion of "refinement."

3. "Catchall" Hypotheses in Sociological Research

To explicate the nature of normal science, we shift gears to define what Salmon (1989) and other philosophers call "catchall hypotheses." Typically, scientists operate on some variation of the formula:

Given conditions C, then X ---> Y (1)

This is our **primary** hypothesis. There are, of course, other possibilities, e.g.:

Given conditions C, then W ---> Y (2)

or,

Given conditions C, then XW ---> Y (3)

etc., etc. This series can be extended indefinitely and, taken as a whole, comprises the "catchall hypothesis." Excluding the stated causes of Y in our primary hypothesis, the "catchall" includes all the other causes of Y, including some that we may suspect as well as others of which we are ignorant. The "catchall" also includes the possibility of measurement and calculation errors, and the possibility that the presumed relation between X and Y is spurious or an artifact of the research process.

In a mature science, the primary hypothesis is well worked out and empirically verified. The probability that the "catchall" is correct is demonstrably nil. The day-to-day work of normal science becomes the methodical extension of basic, agreed upon principles to new areas of investigation and application together with the mopping up of minor discrepancies.

In a mature science, if the research designs, data collection techniques, and/or analytical procedures employed by any given group of scientists do not efficaciously reduce the probability of the "catchall" being correct, then these designs, techniques, and/or procedures are fairly quickly thrown over. This is not always so in pre-paradigmatic situations and ideological rationales may argue for retaining ineffective methods.

4. The "Catchall" and the R^2 Track Record

What needs emphasis is that Salmon's "catchall" is the direct equivalent of "unexplained variance" in regression analysis. Similarly, the primary hypothesis is equivalent to the explained variance or R^2 value.[6] We can ask of

virtually all published quantitative research in the **American Sociological Review**, for example, which has the higher probability of being correct: the primary hypothesis or the "catchall"? This is a simple matter of direct empirical investigation.

Examination of the first half of the 1989 issues of **ASR** shows only two articles where R^2 values reached more than 0.50. Much more typical are R^2s of 0.21, 0.32, 0.14, 0.075, 0.012, etc. In the latter cases, the "catchalls" would have inverse values of 0.79, 0.68, 0.86, 0.98, 0.99, etc. In short, the preponderance of the evidence is that the "catchalls" have a high and vastly unacceptable probability of being correct. This sort of result does not typify a mature science in Kuhn's terms. Statistical sociology today is a great long way from the normal science activity of fine tuning an already highly polished empirico-epistemological system of knowledge.

Interestingly, the **first** use of regression analysis in **ASR**, in 1936, reported R^2s of 0.91 and described this as a "moderate" degree of explanation. This degree of caution quickly eroded, and by 1937, an R^2 of 0.62 was described as a "high" degree of explanation, and in a later issue an R^2 of 0.56 was used in talking about variables that were "closely related." It has been downhill since then, and I have heard sociologists whoop it up when obtaining results in which only 25% of the variance in their data are explained, leaving fully 75% for the "catchall." In short, regression analysis has virtually no scientifically valid explanatory utility in contemporary sociology (with the possible exception of some limiting applications in demography). If quantitative sociologists were rational scientists, they would abandon the unproductive and unreflexive use of linear models and their associated logics of explanation.

All of this would be comedic were it not for the power issues involved in the control of disciplinary resources, journals, and graduate programs by quantitative sociologists. There is, as one might suspect, a material basis for this hegemony and Anthony Giddens (1985) shows nicely how quantitative research directly serves the surveillance interests of the state and is thus highly rewarded. At the same time, there is an resurgence of ideological rationale for neo-positivist research, as witnessed in recent essays in **ASR** by Hubert Blalock (1989) and Randall Collins (1989). There have been many who talk ambitiously of the 1990s as the post-positivist era. I don't believe it for a minute. In the meanwhile, we can continue our efforts (and that includes the work of many qualitative researchers) to rescue sociology from our ideological colleagues and set our discipline on the

path of science. We may not have a paradigm, but we can, as Kuhn notes, make significant contributions to knowledge. It is time, for example, for qualitative observers across the country to initiate multiple, comprehensive studies of what it is that quantitative researchers do and say,[8] and the processes they use to reproduce and bolster an intellectually arid and unproductive approach to what might otherwise become a scientific sociology.

Notes

1. Dan Hoyt of Iowa State University was the quantitative sessions organizer for the 1991 MSS meetings. The difficulties in getting this paper on the program were more than offset, however, by the warm and enthusiastic reception it was given by those who finally heard it in Des Moines. I am especially grateful to Professor Michael G. Lacy of Colorado State University who provided an astute critique of my presentation.

2. The first edition of Kuhn's work appeared in 1962. In the 1970 edition, Kuhn states he is glad if social scientists can find something useful in his work, but that he did not write it with the social sciences in mind. Several authors have taken this as a **carte blanche** invitation to redefine Kuhn in virtually any way they please, but doing so does great violence to the sophisticated logic of Kuhn's arguments about the nature of revolutionary and normal science, **per se**.

3. Ritzer, in an autobiographical reminiscence titled "I Never Metatheory I Didn't Like" (presented 6 April 1991 during the centennial celebration of the Department of Sociology at the University of Kansas in Lawrence, Kansas), told his audience that (1) he was never formally trained as a sociologist or in sociological theory and (2) that the idea to do "a Kuhnian analysis" of sociological theory originated not with himself but was suggested as an organizing principle by a reviewer of a Ritzer manuscript.

4. Mayer Zald spoke as the featured dinner speaker at an ASA-funded conference on social movements held at the University of Michigan in 1988. William Julius Wilson gave the keynote address at the centennial celebration of the Department of Sociology at the University of Kansas in Lawrence, Kansas, on 5 April 1991.

5. Yet another egregious interpretation of Kuhn is found in Stephan Fuchs and Jonathan H. Turner (1986).

6. Although computationally different, R^2 is, in this context, logically equivalent with r^2 and is used throughout this paper to reference both computational outcomes.

7. Given a large number of arguable presuppositions, **American Sociological Review** can be rated the most influential journal in American sociology (cf., **Footnotes**, November 1990, p. 4). **ASR** enjoys a long history as an ostensibly "scientific" journal (cf., Lengermann 1979).

8. Elsewhere (Hill 1984), I have stressed the importance of keeping researchers honest **in terms of the principles to which they freely claim allegiance**. That is, since systematic empiricists loudly claim to be about the business of providing scientifically acceptable explanations of relationships between quantified variables of choice, then **it is fair** to point out that, given several decades to perfect their analytical techniques, they have failed miserably and ought to admit as much. Indeed, at this stage, quantitative sociology remains a good deal more "programmatic" in character than does phenomenological sociology. Workers in the latter perspective have produced hundreds of detailed descriptions, **as promised**. The idea of judging the activities of a social group according to its own professed values is one of the key insights of sociology's first systematic methodologist, Harriet Martineau (1838 [1989]). She applied this methodological strategy with skill and insight in **Society in America** Martineau 1837). On a more contemporary footing, Shulamit Reinharz (1984) usefully shows us how to critique methodological oppression from an experiential perspective [and it is from an experiential view that I now point with much chagrin to my own previous work using the linear model of explained variance, e.g. Hill and Roemer (1977)]. It is, of course, fully possible to launch a number of **external** critiques against the logic of explained variance, as, for example, on the one hand, those by formalists such as Willer and Willer (1973) and Fararo (1989) or, on the other hand, by any number of phenomenologists.

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[Note: Included here are several works not discussed in the text above but which add to what I hope is an on-going discussion, including viewpoints that challenge my own position].

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